XX. *On the different methods of constructing a catalogue of fixed stars.* *By* J. POND, *Esq. F. R. S. Astronomer Royal.*

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IN the present state of practical astronomy, the principal object in a national observatory, such as that at Greenwich, is to define the position in the heavens, of the fixed stars and other celestial bodies, at the moment of their passage over the meridian; and we judge of the perfection of the instruments, and of the skill with which they are employed, by the degree of precision with which this operation is performed.

The very great changes and improvements that have taken place within these few years in the instruments of this establishment, are well known to all persons who have interested themselves in its concerns. The liberality of his Majesty's Government has been literally unbounded; and instruments of unusual magnitude and of most difficult construction, have been executed with a success that has exceeded the most sanguine expectation. Such powerful means entrusted to my care, could not but produce, on my part, a continued anxiety that they should be employed in the most advantageous manner. Being constantly under the necessity of reflecting a great deal on the various possible modes, both of making observations and of deducing results from them, I have insensibly been led to adopt methods which differ very materially from those generally pursued,

and some of these appear to me to possess advantages so decided, that I venture to submit them to the consideration of this Society; at the same time, conscious that the subject cannot excite a very general interest, I shall endeavour to abstain as much as possible from every unnecessary detail, and confine myself to the explanation of the general principles of the method which I propose to recommend.

According to the method hitherto invariably followed in this Observatory in constructing a catalogue of stars, either in declination or right ascension, some one star has been taken as a point of departure, and the positions of all the rest determined by direct comparison with this alone. The declinations were determined by direct measurement with γ Draconis;* and α Aquilæ was chosen as the common term of comparison in right ascension. This mode of proceeding with the mural quadrant, though evidently capable of improvement, was not so very objectionable, as when applied to the observations made with the transit instrument; as the observer must be supposed desirous of obtaining the greatest possible accuracy from a given number of observations.

Indeed, in the latter case, the principle is so very objectionable, that I cannot now help expressing some surprize that it should have been employed for so great a length of time. In the first place, every result deduced from the observation of each star is affected with a double error; that committed in the observation of α Aquilæ, and that in the observation

* This will evidently appear to have been the case, if it be considered that the plumb line in the mural quadrant performed no other office than that of maintaining the instrument in a given position.

of the star itself; this objection however is but trifling, compared with a more important one, which is this: if the observation of α Aquilæ be omitted, either from bad weather, or from its passing the meridian at an inconvenient hour, or from neglect, then the observations of all the other stars are, for the purposes of this investigation, rendered entirely useless. Hence arose the necessity of combining the observations of so many years, to construct a catalogue with the accuracy required. That extreme accuracy was ultimately obtained, I am most happy to have it in my power to affirm, since the difference is almost insensible between the catalogue lately deduced from the new transit instrument and that of my predecessor; though an interval of ten years has intervened between the periods of their construction. The advantage therefore of my method I conceive to consist in this; that a catalogue may be constructed from the observations of a single year, equal in accuracy to one which formerly was obtained in three.

The method I propose is equally applicable to the mural circle and the transit instrument. With neither do I assume any particular star as a point of departure, in preference to the rest; but, on the contrary, every star in its turn is assumed as a point of reference to the others; thus endeavouring, in the first instance, to establish their relative distances from each other on the equator, or meridian, leaving the choice and determination of some common point of departure as a subject for future consideration.

To render this more easily intelligible, perhaps it would be better to consider each instrument separately.

Whoever attentively examines the construction of the mural circle at Greenwich, will perceive that its operation is entirely limited to the measurement of the meridional distance between different stars. It is, in fact, a theodolite placed vertically. By an extension of its principle, it can measure the distance between a star and its reflected image from a mercurial horizon, and thus determine the altitude of a star; and, in common with every other circle, it can measure the distance of a circumpolar star from the pole by an observation of its inferior and superior passage over the meridian. As the instrument cannot be reversed, a plumb line could not be applied, with any advantage. In circles that turn freely in azimuth, a double observation of a star gives the angle which the star makes with the axis of the instrument round which it turns, and a plumb line is properly applied in this case to ensure the verticality of this axis; but on the Greenwich circle, a plumb line could only serve as a point of departure; and this having no reference to the real zenith, would, in my opinion, be very injudiciously chosen.

Let us suppose, by way of example, that twelve stars had been observed with the mural circle, and one of these the pole star; moreover, that this star had been observed below the pole. Then, as it would be easy to determine the polar point, the position of all the remaining stars, with reference to this point, might likewise be ascertained. It is however evident in this case, that each result would be affected by a double error, viz. the error committed in the determination of the polar point, and also that due to the observation of each star.

Let us next suppose that, from some accidental cause, the inferior passage of the pole star had been omitted; then, if the declination of this star be supposed to be known from previous observation, the polar point may be deduced from this supposition; but the pole star, in this case, would have no superiority over any other star, whose polar distance should previously have been determined. If we employ two stars equally well known, it is evident that greater accuracy will be obtained; and it is easy to conceive that by taking a greater number of stars, for instance the whole twelve, we may obtain greater accuracy than even by a double observation of the pole star, because the error of observation, and that arising from false assumption of polar distance, will necessarily be much diminished by the natural tendency which the positive and negative errors will have to counteract each other.

These considerations, aided by daily trial and experience, induced me very soon after the mural circle was erected, to abandon every method which assumed one particular point of departure, and I directed all my efforts to the determination of the difference of declination which existed between every star and all the rest; constructing by this means a catalogue, which, with respect to the pole or the zenith, might be subject to a common error; reserving to myself the power of investigating this common error by a process which will afterwards be explained. In the practical execution of this method, the observation of each star is employed for a double purpose; it first serves, in combination with all the others, to find the common index error; this common error again applied to the individual observation, gives a

new result of the position of the star: and of the totality of these results is the catalogue ultimately formed.

In a paper which I had the honour of communicating to the Society, in the year 1816, I have given several examples of the manner in which the index error of the mural circle is calculated, though I did not at that time explain the principle or advantages of this method. In the printed Greenwich observations will be also found the computations of this index error for every month.

I should observe, that when the greatest possible accuracy is required, the whole of the observations should be re-computed from the catalogue now supposed to be in its most perfect state, except the extremely small improvement it may receive from this process. And thus, by continued observation and approximation, may the *relative* places of the stars be assigned to a very unexpected degree of accuracy. Now, though a continuation of this process has an evident tendency to improve the catalogue, as far as concerns the relative distances of the several stars to each other, yet it can have no effect in correcting any common error that may exist from a false assumption either of the zenith or the pole. I will endeavour, therefore, to explain the next part of the process, which is to determine the common error in polar distance. It is to be presumed that the pole star forms one of the stars of which the intended catalogue is composed, and it should be observed most assiduously, both above and below the pole. These observations are to be treated as if appertaining to different stars, and the place of each determined in the catalogue, by applying the same index error as that em-

ployed for the other stars. When the whole catalogue is completed, these two results are to be examined; and, if they appear equally distant from the pole, the catalogue is affected by no common error; but if, on the contrary, the polar point is not found to be precisely between these results, then half the difference will be the common error.

The details of these computations will be found in the Greenwich observations; and it will there be seen that the polar distances of this star, determined by 158 observations, in the same manner as any other star of the catalogue, namely, by the application of the common index error, was found to be - - - 1.41.21.50

Below the pole, by 132 observations, computed in the same manner	=	358.18.38.32
Their sum - -		359.59.59.82
		360
Difference from 360 -		0 0 0″.18
Difference or common error	=	0.09

This difference, 0″.09, is the common error to be added to each star of the catalogue, and the polar distance of the star thus corrected will be 1.41.21.59.

I find by about 350 observations, deduced in the usual manner, without any reference to the other stars, 1.41.21.65, reduced to the beginning of 1813.

The same principle, which I have thus attempted to explain, may be applied with equal facility, and even greater advantage, to the formation of a catalogue in right ascension. The

error of the clock in this case, answers to the index error of the circle, and is investigated in precisely the same manner. An approximate catalogue is first assumed; and here, as with the circle, the observation of each star serves a double purpose: in the first place, in common with all the rest, it is employed to determine the error of the clock; and the error thus found is applied to the observation of the star. In this case, the right ascension of the star is supposed to be known; in the second part of the process, the star is considered as a planet, or unknown object, and its right ascension found by the usual rules, and recorded as a single result; and from the totality of these results is the right ascension ultimately obtained.

Should the deduced right ascensions differ materially from those originally assumed, the improved catalogue must be substituted for the approximate one, and the whole process recomputed. In this substitution, however, some judgment and discretionary power must be exercised; for should the assumed or approximate catalogue be very exact, and the subsequent observations few or inaccurate, it is evident that the new catalogue might be less correct than the assumed one; this, however, does not arise from any defect in the method, but is the inevitable consequence of any attempt to improve good observations by bad ones. As a practical illustration of this remark, I might add, that having myself assumed an approximate catalogue, so exact as that of Dr. MASKELYNE, it happens that at this moment, in the case of some stars which have not been very frequently observed, I have great doubts whether the new determination is more

exact than the old one; but by continuing this process, a proper ascendancy will necessarily be acquired by the latest observations.

In comparing my catalogue of right ascensions with that of Dr. MASKELYNE, I meet with a singular coincidence, which seems to me to illustrate and confirm, in a very striking manner, the advantage of the principle in question. In each catalogue, the right ascension of α Aquilæ, though deduced apparently by a different process, comes out the same, even to the hundredth part of a second. Accident may possibly have some little share in this very exact coincidence, but it appears to me chiefly to arise from the very nature of our respective processes. In Dr. MASKELYNE's method, the right ascension of every star is derived from direct comparison with α Aquilæ, or in other words, the right ascension of α Aquilæ is derived by comparing it with every star. So it is in my method; and hence the same result ought to be obtained. But the advantage which in one case is peculiar to α Aquilæ, is in the other method extended to every star: no possible reason can be given, why the place of one star should be more accurately assigned than that of another, provided an equal number of observations be obtained of each, since the place of every star is deduced exactly in the same manner from a comparison of all the rest.*

Though not immediately connected with the present subject, I wish to take this opportunity of stating, that, in comparing the observations of the old transit instrument with those of the new one, I find a much less difference than I

* As I have nothing new to offer, as to the method of deducing the equinoctial point from observations of the sun, I have not taken this part of the subject into consideration.

expected. It appears to me that the former instrument must have described nearly a correct hour circle, though this circle was evidently not exactly the meridian. With all the decided superiority of the new instrument, I cannot venture to assign to the catalogue of my predecessor an error much greater than one tenth of a second of time, even in the stars near the horizon, where the error appears probably to have been the greatest. I trust it will be considered by astronomers, as creditable to the history of this Observatory, that two observers, with different instruments (and by as different a method of computation as the case admits of) should deduce two catalogues so exactly alike, that they may be considered almost as identical.

It is an interesting question to every astronomer possessed of a valuable instrument, to know to what degree of accuracy its results can be depended on. I have examined a great number of observations made with the new transit instrument with this view, and it appears to me, that near the equator, 60 observations will generally give the second decimal place of a second of time very correctly; 120 observations will give this with greater certainty, but not in the proportion of two to one.* This I think is rather a greater exactness than can be obtained by the mural circle,† and the reason I apprehend to be this.

* I find, by the rule given by M. LAPLACE, that the probable error of 120 observations is 0″,005 in time.

† The optical power of the new transit instrument is so decidedly greater than in the former one, that each observation must necessarily be more exact; but I do not find the discordances in the ultimate results smaller in the same proportion: from this circumstance I conclude, that the limit to accuracy consists rather in the clock, than in the instrument.

The Greenwich transit clock, compared with others, is, I believe, considered as a

With the mural circle, or any similar instrument, the place of a star appears to be defined at the moment of observation with much greater precision than by a transit instrument; but the error of the latter, I conceive, to be much more purely accidental; so that the result of a great number of observations will be more exact. Local refraction places an insurmountable limit to observations in declination, particularly at any sensible distance from the zenith: observations in right ascension are free from this material source of error, and are therefore susceptible of attaining greater accuracy by continued perseverance.

I have no doubt but to many persons, and even to those extremely conversant with the theory of astronomy, it may appear, that a very undue degree of importance is attached by practical astronomers to the investigation of such minute quantities as form the subject of this paper; and it is a question often asked, of what importance can it be to science, that the place of a fixed star should be so accurately ascertained? A person in my situation might reply, that being employed to do this, it is incumbent on him to do it in the best manner possible, leaving the question of utility to others; but there is really a very reasonable and satisfactory answer to be given to this question. In the first place, since the position of the sun, moon, and planets, are deduced from direct comparison with the principal stars, it is requisite, for this purpose, that their places should be accurately known, and their various changes ascertained. Moreover,

very good one; but of all the instruments in the Observatory, it is certainly that in which improvement would be most beneficial.

though in common language we speak of determining the places of the fixed stars relatively to the equator and ecliptic, the real fact is, that the stars being stationary, it is the situation of these circles themselves, and the point of their intersection, that is the ultimate object of research; and in this point of view we at once perceive how these precise determinations may facilitate the most abstruse investigations of physical astronomy.